yet has been published on this suggestion.* Clearly further research on this problem is required. Investigation must be directed toward identifying the producing mechanism in each case for it is quite reasonable to believe more than one mechanism may exist in the real atmosphere.

The cellular cloud fields Widger discusses in the subsection on low-level winds are exciting because this particular scale of convective cells were not discovered or even suspected until April 1960 when TIROS I pictures were obtained!

These patterns are frequent over oceans but almost entirely absent from land areas. Consequently their nature and even their description are hampered by the lack of data over oceans—especially the measurements of vertical shear and stability. Theory, as well as experiments mentioned by Widger ([1] p. 278), indicates that these variables are critical and that wind speed by itself is less important. For that reason it can be quite misleading to correlate a small sample of cellular patterns with speeds. Further, study of cases in the Southern Hemisphere oceans reported by Merritt is further handicapped by the dearth of actual surface wind reports in the southern oceans. Geostrophic winds derived from surface analysis in the Southern Hemisphere were used where ship reports were not available. The results consequently must be viewed as tentative and preliminary.

A paper just published by van der Ham [3] discusses the interpretation of cellular patterns. He does not consider them as speed indicators but points out their close association with stability and heating of the air by a warm ocean surface.

Meteorologists working in the field of satellite data are coming more and more to realize that there is a large amount of meteorological information in the organization exhibited by cloud patterns. Progress in their interpretation is being made by many organizations, including the group working under Dr. Widger. Many results are no doubt being prepared for publication even as these lines are read. We can look forward to the need for another synthesis, perhaps by the same author, within the next few years.

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REPLY

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Mr. Hubert and I are clearly in agreement on at least two points: (1) the value of summarizing significant areas of our currently existing knowledge, even when the state of that knowledge is far less than is to be desired; and (2) the continuing need for extensive basic and applied research on the significance and interpretations of the various vortical-appearing cloud patterns so frequently photographed by the meteorological satellites.

One source of the dispersion of pressure centers about the apparent cloud vortex centers as shown in figure 13 of [6] derives (as Mr. Hubert is well aware) from the "ground rules" of the study under which these statistics were developed. To quote from the Final Report [1] of that investigation:

"Emphasis was placed on oceanic cases, since it was believed that cloud vortex patterns would appear more clearly organized there and because such regions were felt to be more representative of the other data-sparse areas where interpretations of the satellite observations are particularly critical. In retrospect, the problems encountered regarding the accuracy of conventional analyses over the oceanic areas suggest that this emphasis may have been somewhat unfortunate."

A second source derives from the fact that figure 13 contains what is now known to be a heterogeneity of cases. Work conducted by Rogers, in parallel with the statistical determinations summarized in figure 13, has since clearly demonstrated "that it is possible for spiral cloud patterns to exist when there is no actual closed vortex in the air flow." These findings are discussed in section 5 of [1] and summarized on pages 265–266 and 269 of [6]. They emphasize the need for greater semantic clarity in discussions of these matters since there is no simple one-to-one relationship between the various types of vortical-appearing cloud patterns and the hydrodynamically significant pressure vortices. An obvious additional implication is the need for improved and objective ways to distinguish between such cases as those studied by Rogers and the "clearly definitive cloud vortex" discussed on page 272 of [6].

The paper [6] discussed by Mr. Hubert was clearly stated as "an attempt to integrate existing published knowledge" and made no pretense of providing a final solution to these problems. But quoting again from [1]:

"These studies have re-emphasized the complexity and variety of cloud vortex patterns, and our serious lack of anything approaching an adequate understanding of what various vortices really signify. While many vortices appear clearly associated with the development sequence proposed by the Boucher-Newcomb model (although not always starting with the initial stages of that model),

other vortices appear to be of distinctly different origins and synoptic associations. Until basic understandings of these differences are considerably improved, there will continue to be strong limitations to attempts to derive reliable and unambiguous synoptic and operational interpretations. Some progress along these lines has resulted from [these studies], but these investigations have only scratched the surface of the real problems."

As Mr. Hubert is aware, there exists considerable interest in, and specific proposals for, conducting such studies if the resources indispensable to their support can be provided.

Some very recent results further indicate the complexity of the problem, especially when they are added to such earlier suggestions as Mr. Hubert's vertical wind shear concept. Holl et al. [2] have demonstrated that the deforming effects, on pre-existing cloudiness, of nonuniform advection almost certainly are significant, as might have been surmised from the work of P. Welander as discussed in [4].

On the other hand, Rogers, in some soon to be published results [7], has used some computations of Sanders [5] to demonstrate that the key features of one type of vortical cloud pattern correspond to the pattern of dynamically induced vertical motions, and to infer that the cloud patterns noted in other stages of system development may well be explainable in terms of such mechanisms. The known asymmetry of vertical motion patterns thus provides on explanation of the lack, in some cases, of coincidence between cloud and circulation centers.

With regard to the derivation of low-level wind information from cellular cloud patterns, these results were specifically stated as being "tentatively devised." As implied in [1] and specifically stated in the original report [3] on these studies, the cases chosen were from situations in which low-level wind would be closely related to one of the variables, vertical shear, which Mr. Hubert accepts as critical to the cloud pattern. Furthermore, [3] specifically states: "Each example [has] a related surface wind report either from a station within the area of the photograph or from a time-translated related surface report." Contrary to Mr. Hubert's statement, geostrophic winds were *not* used. And the widely accepted concept that these cellular clouds are closely associated with stability and heating of the air by a warm ocean surface does not preclude their larger-scale patterns also being indicators of approximate wind velocities.

Mr. Hubert will be pleased to learn that a synthesis of published interpretations of a much wider variety of satellite-observed cloud patterns will soon be available [7]. And while we sincerely hope this and the paper [6] discussed by Mr. Hubert will merit modification within only a few years, such a situation will require: (a) That competent research meteorologists maintain and/or acquire a vital interest in the study and interpretation of meteorological satellite data. (b) That good quality satellite data, processed through the stage of geographical referencing, be readily available to all such scientists. (c) That resources adequate to the support of such studies and scientists be made available to minimize diversions of competent personnel to other fields of endeavor.

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